THE PRICE EFFECT OF IMPACT FEES ON NEW, SINGLE-FAMILY HOUSING: AN EMPIRICAL STUDY

By

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This research investigates the economic incidence and price effects of impact fees on new, single-family dwellings in selected cities in Pinellas County, Florida, from 1970 to 1984. Five cities were chosen for study. Two cities imposed impact fees of at least $1,000 per single-family dwelling, one for a period of nine years and the other for a period of two years, while the other cities imposed no impact fees. The methodology employed uses the data on new housing in the five cities not subject to impact fees to predict the price of housing in the two cities subsequent to implementation of impact fees. The predicted price is obtained by using the hedonic method of regressing actual sale price on a small subset of the individual characteristics comprising the new dwelling.

Using a dataset of 10,294 new home sales, the model consistently underestimates actual sale price by approximately 3 percent. This led to the conclusion that impact fees were contributing to higher new home sale prices and that builders were passing on the incremental cost of impact fees to homebuyers.

There is also evidence that builders stockpiled construction permits prior to enactment of an impact fee ordinance in order to avoid the costs associated with fees. A chi-square test was conducted and the results support this observation.

CHAPTER I
INTRODUCTION

Problem Statement

Local government officials in growth states are in a dilemma. Increasingly, they are confronted with the problem of providing new infrastructure to service growth and development while deferring maintenance and repairs on existing facilities. Moreover, their ability to raise revenues through property taxes is being severely constrained by legislation supporting tax and expenditure limitations (Dowall, 1983). In addition, the current administration in Washington has scaled back federal revenues and grant programs intended for state and local capital improvement projects. Local officials are faced with the difficult task of paying for new public services while at the same time experiencing a declining revenue base. In light of this situation, local governments are looking to those individuals and firms creating the need for infrastructure improvements to underwrite the cost.

One financing mechanism gaining popularity in Florida is the use of development exactions. A considerable controversy has evolved over the actual effect of exactions, and in particular impact fees, on the price of new housing (Welch, 1986). This research project examines the effect of impact fees on the price of new, detached, single-family dwellings in one metropolitan housing market in Florida.

Significance and Justification

From 1970 to 1980, Florida's population grew by almost three million people, more than any other state except California and Texas. The state's growth rate of 43.5 percent was also the third highest of any state behind that of Nevada and Arizona. Growth has continued at a rate of 15.7 percent from 1980 to 1985, or about 300,000 persons annually (Smith, 1987). Almost 89 percent of the increase is due to net migration with the remainder due to natural increase (Israel & Beaulieu, 1986). Growth has not been evenly distributed around the state. According to Israel and Beaulieu, nearly 80 percent of the state's current population reside in coastal communities. In light of the economic viability and political resurgence of the Sunbelt states, it is unlikely that this growth will subside in the immediate future. It is almost certain that major growth areas of Florida will experience increasing demand for more and better public services in the years to come.

Local government officials already encountering budget shortfalls due to Federal cutbacks will face, and indeed already are facing, significant problems in providing the new infrastructure to meet the needs of rapid development.

A recent study conducted by the University of Florida's Bureau of Economic and Business Research concluded that Florida will need more than $47 billion in new and improved roads, sewers, schools, water treatment, and other public facilities by the year 2000 (Welch, 1986). However, at the same time demand for new public facilities is increasing, the ability of the state to pay for these facilities is decreasing. The chairman of the 1986 Senate appropriations committee, Mr. Patrick Neal, calculated that the tax burden had dropped from 4.3 percent of real gross personal income in 1970 to 3.7 percent in 1985. The implication is clear--Florida has fallen further and further behind in meeting the infrastructure needs of a rapidly expanding population (Koenig, 1986). Indeed, "it was not until 1985 that [state leaders] finally developed a consensus that growth, at least under Florida's current tax structure, does not pay for itself" (Welch, 1986, p. 74).

In order to address this situation various proposals have been debated in the popular press and in recent Legislative sessions in Tallahassee. Increasing ad valorem property taxes, implementing a state income tax, legalizing casino gambling, and a sales tax on services are frequently discussed, potential solutions to the growth-induced infrastructure problem. For obvious reasons politicians are hesitant to push too strongly for a tax increase of any kind, especially one to underwrite the costs associated with new growth. And with public sentiment in certain parts of the state strongly against gambling, even when the proceeds are to be used for public education, the likelihood of this becoming a viable solution remains doubtful. Although a sales tax on services was approved in the 1987 Legislative session, its future is nebulous since opponents are gaining support in a national campaign to repeal the tax.

Equitable pricing of public services is one of the key issues surrounding the provision of capital improvements to accommodate development. More specifically, the controversy centers around who should pay the costs of capital improvements when those benefiting are primarily residents of the new development. Current state residents in many cases are reluctant to pay for the new roads, sewers, and recreational facilities required by the over 800 new residents arriving daily in Florida (Smith, 1986). The prevailing sentiment appears to be to let those who benefit most from new public services pay the costs (in excess of the revenues generated by the project) of those services (Seidel, 1978).

The belief that growth should "pay its own way" has evolved from realization of the negative fiscal impacts of new residential development. Essentially the problem is one of additional costs, caused by development, which exceed the additional revenues, or benefits, associated with that development. Since the required public services, or capital improvements, are, in most cases, priced on an average cost basis rather than on a marginal cost basis (Sonstelie & Gin, 1982), the result is a negative fiscal impact; a situation where costs are greater than benefits to the general public.[1]

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[1] This assumes the capital improvement is at or beyond its point of optimum capacity--the average cost curve is rising and marginal cost is greater than average cost. Average cost is the total cost of providing some public service divided by the number of units, in this case dwellings, receiving that service. Marginal cost is the change in total cost of supplying an incremental, or additional, unit of that service.

Basic price theory holds that the average cost curve, consisting of fixed and variable costs, is typically U-shaped, at least in the short run. As output, or quantity of service provided, is increased, the variable factors of production, like labor, will increase more or less proportionally up to some capacity level of output determined by the amount of fixed factors. At this point, more than a proportional amount of the variable factors is needed to increase output, or quantity of service provided. The variable cost function which was increasing at a proportional rate as output increased will now increase at a rate more than proportional to output and take on a steeper slope. Combining the average fixed cost curve and the average variable cost curve results in the U-shaped average cost curve (Varian, 1984).

Price theory further states that the average cost curve will be intersected from below at its minimum point by the marginal cost curve; marginal cost equals average cost at the point of minimum average cost. The relationship of the marginal cost curve to the variable cost curve is such that the marginal cost curve lies below the average cost curve where the average cost curve is decreasing, above it when it is increasing and, therefore, passes through the minimum point. (For a formal proof see Varian.)

In the long run it is assumed that all costs are variable and that increasing average costs are not reasonable. Because a firm could always replicate its production process a constant or decreasing average cost function would be likely (Varian, 1984). However, in the case under consideration--the provision of capital improvements by local government--it may not be a reasonable assumption that all factors of production, and thus costs, are variable. There may be plant and equipment that cannot be replicated at any cost and, therefore, are essentially fixed in the long run. In this

Since public service costs traditionally have been levied on existing residents, it is understandable that communities are opposed to development that does not cover its costs. This conflict has been a major factor in the development of the present "let those who benefit pay" (Nicholas, 1985) system of infrastructure financing, a system of public finance funded by those for whom capital improvements are provided.

It should be noted that existing residents also can, and often do, benefit by growth and from the infrastructure provided to service development. For instance, a community will experience a broader property tax base from the addition of new properties to the tax roles. This could have the effect of reducing ad valorem property taxes. Additionally, many of the capital improvements provided for development will benefit the general public by reducing social costs through improved efficiency in public services. New roads which alleviate congestion both from new development and too many existing users benefit all residents, not just those moving into new homes. In the literature review that follows no study or discussion of significance was found dealing with this aspect of infrastructure finance. Frieden (1980), however, does allude to the fact that the community as a

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situation the firm or local government would not benefit from the long run constant returns to scale technology and thus continue to face the U-shaped average cost curve. The analysis in the long run with some factor(s) of production fixed would follow that of the short run.

whole, as well as residents of new homes, benefits from offsite infrastructure expenditures which are funded with development fees.

In support of infrastructure financing systems which advocate, either implicitly or explicitly, the theory of marginal cost pricing, Stegman (1986) stated that

> Current efforts to make new development responsible for more of the public infrastructure that serves it have ushered in a new generation of development regulations. Hundreds of rapidly growing communities have adopted or are considering adopting various forms of private financing--including exactions and development fees--to construct at least some portion of new offsite infrastructure. In so doing, they are attempting to shift more of growth's capital costs to developers and "new" residents. (p. 2)

Of the various forms of private financing, cash payments, or impact fees, are increasingly being used by local governments to finance the cost of new capital projects. Impact fees are assessed by local authorities against development in order to pay for the cost of public services necessitated by the development. Sewers, water mains, park facilities, fire and police protection, roads, and other public services are all currently funded with impact fees.[2]

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[2] Impact fees are promulgated on the basis of two arguments. First, a system of impact fees "represents an attempt to charge the users of urban services the true cost of extending such services" (Landis, 1986, p. 12), to new development. Second, impact fees as a tool of growth management, or land use policy, represent "a purposeful attempt to shift development away from the urban fringe, and back inward to fill vacant parcels in previously developed areas" (Landis, p.12). This research does not attempt to differentiate between the two as there is nothing to indicate whether an impact fee based on one or

A survey reported in April 1986 found 15 counties and 62 cities in Florida using impact fees to pay for one or more of the services mentioned above (Miller & Lines, 1986). In addition, 13 cities and 17 counties were either acting on or had impact fee ordinances pending approval. According to Miller and Lines,

> The use of impact fees in Florida proliferated following the landmark decision in Builders and Contractors of Pinellas County v. City of Dunedin, 329 So. 2d. 314 (FL 1976), which established the criteria that impact fee ordinances must meet in order to be upheld legally. As a result of the Local Government Comprehensive Planning and Land Development Act of 1985 providing explicit statutory authority for the enactment of impact fees, their use can be expected to increase further. (p. 1)

However, not all parties to the debate focusing on the allocation of impact fees agree that those who occupy new housing should be the ones to shoulder the burden of impact fees. The president of the Florida Homebuilders Association, Sanford Miot, was quoted as saying, "rather than make the gutsy decision and say we ought to battle this and be fair, the state took the path of least resistance and taxed the one group without representation--the new homebuyer" (Welch, 1986, p. 74). Adding further support to this position, Gary Kendall, a West Palm Beach builder, said "they [state legislators and local government officials] are going to make affordable housing obsolete" (Welch, p. 74). This would seem to indicate that builders and developers are passing along the cost of

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the other of the two premises will result in any measurable difference in new house prices.

impact fee assessments in the form of higher prices or are moving to upscale (higher price) housing construction where profits are reputed to be greater. The result is higher prices for new homes in all market segments and possibly fewer homes built in the lower price ranges, further driving up the price of what housing is built.

The argument offered by builders asserts that many new homebuyers are current residents of the state and already have paid their fair share of necessary infrastructure costs through property taxes, special assessments, and user fees. Furthermore, people just moving to Florida often buy and occupy used homes rather than building a new dwelling. The implication is that the new resident to Florida will frequently receive a subsidy from existing residents who elect to purchase a new home.

It should be clear one of the main questions stemming from the previous discussion is the identity of the person actually paying impact fees. Several researchers recently have looked at this question (Downing & McCaleb, in press; Fischel, in press; Stegman, 1986; Weitz, 1985) and have found that the answer is not as straightforward as commonly thought. The incidence of impact fees can fall in whole, or in part, upon the consumer (new homebuyer), the builder, or the landowner from whom the land was originally purchased. It is also possible that other parties such as existing homeowners who are not buying a new home may be underwriting some of the costs associated with providing capital improvements. For instance, residents may have their property taxes raised to supplement impact fee collections in order to construct the new elementary school needed to educate additional students who have moved into the community. This could easily be the case in counties or cities that assess impact fees for schools but find more money is needed. Existing residents now find they are paying a portion of the costs caused by growth.

For these reasons, this research project is designed to examine more closely the incidence of impact fees in Florida and their effect on new housing prices.

Definitions

1. Developer exactions--Required payments either in kind, such as land dedications for parks or green space, or cash assessments, levied to compensate for the capital costs of development.

2. Impact fee--A charge imposed by local government on incremental units of growth to pay an equitable share of the increased public capital costs necessitated by that growth; a form of developer exaction, and sometimes referred to as a development fee.

3. Local government--Any county or municipal ruling body freely elected and serving a specified term, or designee appointed or hired by said body to act in

accordance with the rules and regulations of the county or municipality.

4. Dwelling--A detached, single-family structure in which one or more persons lives.

5. Developer/Homebuilder--Any person or persons engaged in the procurement of raw land, obtaining necessary approvals and permits, adding the infrastructure needed to produce finished lots, and construction of residential living facilities. In this study the terms are equivalent in meaning.

6. Price elasticity of demand--The percentage change in quantity purchased divided by the percentage change in price.

CHAPTER II
THEORETICAL FRAMEWORK

Literature Review

Not only is the total body of literature devoted to developer exactions sparse, extant studies primarily have focused on the legal and planning aspects rather than the economics of the issue. As Downing and McCaleb (in press) have noted, "economists have not been major contributors to the discussion [and] thus, economic analysis of exactions [and in particular impact fees] is limited." Stegman (1986) has stated that while "economic, political, and social factors have been the dominant forces in creating the discontent with traditional forms of public financing, [it is] legal considerations . . . [that] have largely determined the extent and applications of private financing" (pp. 2-3).

In order to ascertain the effect of impact fees on the price of new dwellings it is necessary to determine the incidence of the fee, or the identity of the parties ultimately paying the cost of the offsite infrastructure, and the magnitude of those costs. The literature is not in agreement as to who really pays the cost of impact fees.

A study conducted by the U. S. Department of Housing and Urban Development's Task Force on Housing Costs reported in 1978,

> That local governments are steadily transferring from the community at large to the developer, and thence to the new housing consumer, a greater share of the public capital costs of growth. This is being done through the imposition of fees and charges as well as through requirements for construction and dedication. (p. 25)

Ellickson (1982) asserts that developers,

> When bidding for land in a city with [high development fees] would bid less, because they would take into account the fee they would later have to pay when they built. The burden would thus be passed backward to the people who, at the time the fee became fully anticipated, owned land suitable for residential development. (p. 142)

Downing and McCaleb argue that in the long run and in a competitive market for development sites (a market where there are alternative sites available and in which no site has unique features sufficient to impart a value differential) the incidence of impact fees will likely fall in whole, or in part, on the landowner. This situation occurs when the costs of the public services to be provided are greater than the actual or perceived benefits to the consumer for whom the improvements are provided. If the actual or perceived benefits are greater than the costs, then the developer will be able to recover the amount of the impact fee from the homebuyer through a higher sale price. Otherwise, Downing and McCaleb contend, the developer will estimate the amount of development fees required of him prior to construction and will capitalize that amount into a lower bid price for the parcel desired.

A key assumption of the Downing and McCaleb study is that "the market for development sites is competitive" (in press). If, however, a developer does not have alternative sites to choose from or if a particular site is markedly preferable to other sites, then a degree of monopoly power is introduced into the development process. Ordinarily the developer would be able to charge the consumer a higher price for a given dwelling commensurate with the increased desirability of the site. This fact is not lost on the original landowner who will demand a higher price for his property from the developer. It is not clear how much, if any, of the impact fee is passed backward to the landowner through a reduced bid price. The literature does not provide any empirical evidence on this issue although Downing and McCaleb assert that any monopoly profits are redistributed from the landowner through the developer to existing residents.

There are other cases where the developer may not be able to shift the incidence of impact fees back to the landowner. In Florida, where hundreds of thousands of vacant lots were platted prior to 1970 (Jurgensmeyer, 1985), developers may have to bear totally the incidence of impact fees in the short run. Here short run is the time it takes to build and sell a home once the building permit is granted. The developer, or homebuilder, will try to capitalize the costs into a higher asking price but may not be successful if potential buyers have alternate housing choices. In this situation a developer may find he absorbs the costs, in whole or in part, through reduced profits.

Another situation that clouds the issue of incidence results when a developer underestimates the total impact fees required and offers a bid price that is too high to recover his additional costs. In this case "he will indeed bear a portion of the exaction costs resulting from his underestimation" (Downing & McCaleb, in press). This could happen when the development fees required are determined by negotiation with local authorities rather than through set fee schedules. A developer may seriously underestimate the fees required for a house to be built months or years in the future.

Stegman (1986) has stated that

> Although considered an almost universal truth, the view that homebuyers always bear the full incidence of development fees accurately portrays conditions only in a particular kind of market--communities that are much more attractive places in which to live than other nearby locations. In these highly desirable markets housing demand is likely not to be sensitive to price hikes. Therefore, a substantial fraction of development fees . . . can be simply passed along to homebuyers. (p. 4)

The market described above is a situation where the price elasticity of demand is low--the type of market which economic theory indicates will cause consumers to pay higher prices with very little, if any, reduction in quantity purchased. The assumption is that there are no acceptable substitutes for the good in question. In this case prospective homebuyers consider the desired community preferable to other communities and are willing to pay a premium, the development fee, to live there. Other communities may be comparable in terms of amenities, location, and socio-economic status, but if consumer demand to live there is lower than the demand in a more desirable community, the proportion of development fees able to be passed on to homebuyers will be less. Here the price elasticity of demand is higher indicating consumers will purchase less of a product, in this case housing, for a percentage increase in price.

Weitz (1985) considered the supply and demand elasticities of various submarkets in his discussion of the incidence of impact fees. He concluded through means of four contrived supply and demand scenarios that the person paying the impact fee will depend in great part on the submarket in question. If submarkets are such that the elasticity of supply and demand is roughly equivalent, then the buyer and seller (assumed to be the builder) will share the cost of impact fees assessed against that structure. In a situation where demand is relatively inelastic and supply is highly elastic, which Weitz says, "according to the literature is rather typical" (p. 94), then the buyer can be expected to assume most of the impact fee charge. The builder can be expected to bear most of the cost of infrastructure charges when demand is highly elastic and supply is relatively inelastic, a situation Weitz claims, "is more likely to be a theory not found in nature" (p. 89).

Fischel (in press) examined the issue of impact fees from the standpoint of economic efficiency and then looked at the incidence question. According to Fischel, "the incidence question is often the most pressing issue, but this question cannot be answered without first addressing the efficiency issue." If the system of exactions is efficient, the developer will pay the cost of the exactions. If the system is not efficient, the developer will pass along in whole, or in part, the cost of the exaction, or development fee, to the homebuyer.

Utilizing a property rights approach in his analysis of exactions, Fischel's major and novel assumption is that local government is the potential owner of the rights associated with real property development. The developer, or homebuilder, must pay to the local authority a price, the development fee, for the privilege of constructing a dwelling on a particular parcel of real estate. As such, the owner of the development rights may be able to sell them to another party who values the rights more. In this case that party would be the developer.

The distinction typically made by other authors between onsite and offsite infrastructure costs is abandoned by Fischel. Instead, costs are either internal or external to the project. Internal costs are absorbed by the developer and passed along to the buyer just as costs for timber, labor, and other inputs are incorporated into the offering price. External costs are those not directly associated with the actual physical property but rather fall to some third party.

Fischel allows that a system of exactions keyed to the location of expenditures is not as efficient as one which can provide direct remuneration to third parties for external costs imposed upon them by development. Efficiency is increased because affected parties may be willing to accept less in the form of a direct payment than the amount required of the developer for the provision of additional capital improvements.

Fischel concludes that the existing system of exactions is not efficient and the identity of those responsible for development fees is not certain. This inefficiency will cause housing costs to rise and will foster sprawl in future development. The outcome will vary depending on the relative efficiency of the system.

In the discussion thus far, the questions of incidence, equity, and efficiency of impact fees closely parallel the questions raised whenever a new tax is proposed. In fact, several authors have determined that exactions, and in particular impact fees, are essentially a tax on development (Downing & McCaleb, in press; Ellickson, 1982; Weitz, 1985; Weschler, 1986).

The tax literature contains a body of research focusing on the controversy about the effects of a development value tax (DVT), sometimes referred to as a development gains tax. The DVT is a one-time levy on the incremental value of land due to rezoning. This work is relevant to the current controversy surrounding impact fees for its attempt to ascertain who ultimately pays the DVT and its effect on the price of land. Rose (1973) summed up the major points of the debate in his paper entitled The Development Value Tax. According to Rose,

> Some argue that the tax causes a higher price of land, a reallocation of resources among alternative uses, and a redistribution of wealth. Others hold that the DVT is like a lump-sum tax, having no effect on price or the allocation of resources. F. G. Pennance (1967, pp. 42-43), for example states, . . . 'A tax on development value in land will reduce the quantity forthcoming at any price', drive . . . 'a wedge between the demand-price and the supply-price of development land . . .', and cause an increase in land price to buyers. M. Neutze (1970, pp. 324, 326) argues that because of a price inelastic demand for urban land, . . . 'most of [a DVT] will be passed on to buyers of developed land.' On the other hand, E. J. Mishan (1970, p. 65) holds that . . . 'a tax on this capital gain of development land [due to rezoning] has no effect whatever on its market price. (p. 271)

Rose assumes "the price of land, as a factor in the production of urban services, is determined predominantly by the demand for housing and stock of housing" and because the DVT does not impact on price through the demand function, "if the tax has any effect on the price of urban land, it must be [through] the supply of annual additions to the stock of housing" (p. 272).

Specifically, the price impact, if any, must be through the rezoning and development choices.

In Rose's analysis the DVT may be collected either at the time of rezoning or at the time of actual development. Considering the latter option first, Rose found that a DVT assessed at the time of development yielded an indeterminate effect: the tax could either accelerate or delay the timing of development. The ultimate effect was conditional upon the state of the urban land market at the time.

A DVT levied at the time of rezoning has no effect on the timing of development for it is a predevelopment decision and therefore has no effect on the stock of housing or its price. However, if the DVT is assessed at the time of rezoning it may in fact affect the timing of the rezoning decision. If the development decision is not independent of the rezoning decision, the tax may have some impact on subsequent prices of housing through its influence on development timing. The timing of development could be affected indirectly through the influence of the DVT on the timing of the rezoning decision.

Rose (1973) depicts three scenarios:

1. The developer decides whether and when to develop and seeks necessary permission of local government which is tantamount to deciding whether and when to rezone;

2. Local government decides when to rezone independent of any action by or preference of the developer and;

3. There is joint action between the developer and local government. The developer prefers an earlier rezoning decision to a later one. Because of his preference the developer will incur some cost to increase the liklihood of a favorable decision.

In this analysis and in the analyses of later researchers, the problem is addressed mathematically as one of maximizing the current value of a piece of land where the variable to be determined is the optimal time of development (Evans, 1982). It is assumed that future income streams to the land, either developed or undeveloped, are known. Thus, any tax which accelerates development will increase the supply of land and reduce the price. Conversely, any tax which delays development will reduce the supply of developable land and increase its price.[1]

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[1] The general form of the expression found in this literature expresses the present value of land, V, as,

$$V = {}_0\int^D f(t)e^{-rt}dt + {}_D\int^\infty h(t,D)e^{-rt}dt \qquad (1)$$

where $f(t)$ = income from predevelopment use
$h(t)$ = income from postdevelopment use
t = time
D = time of development
r = appropriate rate of discount

Alternatively, (1) can be rewritten as,

$$V = F(D) + H(D)e^{-rD} \qquad (2)$$

where $F(D) = {}_0\int^D f(t)e^{-rt}dt$

Rose concludes that in the first two cases a DVT levied at the time of rezoning has no effect on additions to housing stock. In both situations the development decision is a posttax decision and is not affected, either directly or indirectly, by the timing of the rezoning decision.

In the third case, the DVT will cause a rise in house prices through a reduction in annual additions to housing stock. This is because the developer must now incur some cost to persuade local authorities to rezone. Associated with this cost is some probability that his efforts will succeed. The optimal probability is where the marginal value of probability is equal to the marginal cost of probability. The levy of a DVT reduces the "net-of-tax

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and $H(D) = {}_{D}\int^{\infty} h(t,D)e^{-r(t-D)}dt$

With a tax (2) becomes

$$V = F(D) + H(D)e^{-rD} - \tau[H(D) - \phi]e^{-rD} \quad (3)$$

where $\tau.100$ = percent tax rate, $0 < \tau < 1$
ϕ = prezoning purchase price
$H(D) - \phi$ = tax base.

The first order condition for optimal development time is

$$dV/dD = \{f(D) + (1 - \tau)[-rH(D) + H'(D)] - \tau r\phi\}e^{-rD} = 0$$

This implicitly gives D as a function of τ.
Differentiating with respect to τ yields

$$dD/d\tau = \frac{r\phi - rH(D) + H'(D)}{f'(D)+(1-\tau)[-rH'(D)+H''(D)]},$$

The sign of $dD/d\tau$ states the effect of the tax on development. If $dD/d\tau$ is > 0, the tax will delay development. If $dD/d\tau$ is < 0, the tax will accelerate development. If $dD/d\tau$ is = 0, the tax will be neutral.

tax" probability. Therefore, a DVT reduces the "net-of-tax value of the desired outcome, and the marginal value of probability" (1973, p. 273). The marginal value of probability with a tax is less than the marginal value of probability without a tax. Thus, the optimal probability with a DVT is less than the optimal probability without a DVT and developers will reduce their effort to seek a rezoning decision. The result is that government rezones at a rate less than what they would have had the developer engaged in a more active lobbying effort. This has the effect of keeping more land in nondevelopment use and consequently less land in urban use. Therefore, the price of urban land will be higher than it would be had there been no tax.

Several authors have commented on and critiqued the conclusions reached by Rose (1973). Neutze (1974) contends that if property development is competitive, any of the DVT not shifted forward to the homebuyer must be shifted backward to the landowner by developers offering to buy raw land at a lower price. Neutze doubts that property owners will accept much, if any, reduction in the price of their land, particularly if the market is experiencing rising property values in general. He concludes that if prices of raw land do not fall, developers will be unable to buy all the land they desire, the supply of land will be reduced, and the price of housing will increase such that a part of the DVT will be passed on to the homebuyer (p. 92).

Neutze also claims that a DVT levied at the time of development "has no effect on the timing of development, nor on the price of developed land" (p. 91), whereas Rose concludes the price effect is indeterminate. Neutze reaches his conclusion based on his interpretation of Rose's tax as the difference between the value of land in agriculture and the value of land at the time of development, or subsequent to rezoning.

Neutze contrasts the tax assumed by Rose, which is proportional to capital gains, with a lump-sum tax, paid only when the capital gain is realized. Rose assumes the DVT is paid whenever it is levied--at rezoning, development, or sale. Neutze claims the lump-sum tax will cause a delay in development and a rise in the price of new homes reflecting a portion of the tax passed forward.

Foster and Glaister (1975) also question the assumptions and conclusions of Rose (1973). They point out that the term "development gains tax" is a misnomer, noting that the usual case for levying a development gains tax is when there is a discontinuous jump in the value of land because of rezoning or the provision of public services. The gain that is taxed is primarily a result of some action by the local planning authority and not from the actual development as claimed by Rose.

Foster and Glaister dispute the conclusion drawn by Rose when a developer engages in lobbying the local public authorities with the intent of securing an earlier rezoning decision. Rose concluded lobbying is not a costless activity, as assumed by many researchers, and would therefore cause developers to exert less of an effort than they otherwise would, resulting in a lower supply of developable land and higher prices for new housing.

Foster and Glaister extended Rose's analysis and noted that local planning authorities may recognize that a decision to rezone and allow development will benefit the community through the proceeds of the development gains tax. This point was not raised by Rose or Neutze. Therefore, the impact of the tax is indeterminate with the possibility that it will either delay or accelerate the rate of development. This will depend on the relative magnitudes of the private marginal cost and the social marginal benefit.

Rose made the assumption that a DVT might be paid prior to being realized. Foster and Glaister contend that while Rose may have been correct in arguing that a DVT will not delay development if it is levied when permission to develop is secured, he failed to make explicit a crucial assumption. That assumption was that if gains are to be taxed prior to being realized, then the tax liability should be discounted by the private opportunity cost of capital. Otherwise, the tax is at a rate greater than the nominal rate. In this case the development gains tax could hasten the timing of development.

The analysis conducted by Foster and Glaister assumes the development gains tax is levied on the present value of that land use which results in the land being put to its most profitable use, not on the land use predicated upon a particular development. In the terminology of real estate appraisal, the tax should be levied on the value of land in its "highest and best use" and the highest and best use can be achieved instantaneously. Foster and Glaister incorporated the assumption that "the buildings [and other improvements] could be demolished and replaced by ones more suitable to prevailing circumstances at some future date" (p. 215). Therefore, actual returns to land after development will be independent of the original date of development (Downing & McCaleb, in press).

Finally, Foster and Glaister conclude that if the optimal time to develop is not coincident with receipt of planning permission, then a development gains tax will delay development. However, development would occur immediately following rezoning if the income from the current use is less than the net-of-tax development income and provided the tax is less than 100 percent of the excess of postdevelopment income over predevelopment income.

Skouras (1978) found that in a static framework a land tax is perfectly neutral, affecting neither the operating nor investment decisions of the entrepreneur. The tax will, however, reduce the total net returns and consequently reduce the price that can be paid for the land. In this situation the tax is capitalized into the price of the site.

Noting that the landowner's function is not passive with regard to developing his land, Skouras analyzes the optimal timing of development with and without a tax. In the absence of a tax, land should not be developed if its rate of appreciation is greater than its holding costs. Development should take place when the rate of appreciation in land value equals the interest rate. If there is positive income resulting from the land in nondevelopment use, the optimal time of development will be when the rate of appreciation equals the interest rate less the proportion of nondevelopment income to the value in development. If income is increasing, the timing of development will be delayed unless the development value is increasing at a proportionately higher rate.

Skouras then analyzes the situation where a development value tax is levied while assuming "the tax is paid at the time of development" and that it is based on the "difference between the value of land at development and its value just before permission for development (rezoning) [is] granted" (p. 119). In contrast to Rose

(1973) and Neutze (1974), Skouras concludes that the effect of the tax is not indeterminate (Rose) or that it has no effect (Neutze), but that it will accelerate land development. The effect of hastening the optimal time of development is to increase the supply of land and reduce the price of land to developers.

Evans (1982) also analyzed the effect of a development gains tax on the timing of development and its subsequent price effect. He found that a tax levied at the time of development (a pretax decision) will cause a delay in the timing of development whenever "the 'prezoning purchase price' is less than the capitalized value of the current income from the developed land 'in perpetuity'." (p. 61) Development will be hastened in the opposite case and unaffected if the prezoning purchase price equals income from developed land capitalized to perpetuity.

Evans claims this result is implicit in the work of Rose (1973) although not derived by Rose and stands in contrast to the result found by Foster and Glaister (1975). The conclusion proffered by Skouras (1978), that development will always be accelerated in this situation, is not absolutely correct and results from a mathematical error, according to Evans.

The explanation offered by Evans for the seeming discrepancies in the conclusions of Rose, which are essentially the same as Evans, and Foster and Glaister, is "the authors appear to be making different assumptions about the date at which the tax due is paid." (p. 62) Rose assumes the tax is paid at the time of development while Foster and Glaister assume the tax is paid upon rezoning or at subsequent sale prior to development. Given this is true, the tax could have seemingly opposite effects on the timing of development depending on which set of assumptions is chosen.

Evans contends, however, that the tax formulated by Foster and Glaister is really nothing more than a lump-sum tax paid prior to actual development. Therefore, the developer will choose the date of development that he would have chosen originally in the absence of the tax. Thus, the tax has no effect on the timing of development, "provided the tax has to be paid before or at the date at which development would have taken place in the absence of the tax." (p. 63) If the tax is levied after the optimal date of development, payment of the tax would result in immediate development.

In the case where a development gains tax is based on the difference between predevelopment and postdevelopment use and paid at the time of development, Evan's work confirms the conclusions reached by Rose and Neutze. The tax effect is neutral leaving price unaffected and the timing of development unchanged.

Literature Summary

It should be obvious from the literature review that researchers and theoreticians are not in agreement as to the price effects of impact fees on new housing. Arguments can be stated supporting the contention that the cost of impact fees will be passed backward to landowners, while equally persuasive arguments can be proffered supporting the opposite view--that the cost of impact fees will be borne by new homebuyers. Most of the works cited, however, agree that the ultimate effect will depend to a great degree on the relative supply and demand elasticities in the market being studied.

Another question arising from review of the literature on development value taxation is the extent, if any, to which impact fees affect the timing of housing construction. While not the focus of this study, analysis of the data to be used in this research may provide some evidence indicating impact fees are influencing builder decisions regarding the timing of construction.

Theoretical Background

To determine the effect of impact fees on the price of new housing, the following discussion is based on the premise that an impact fee is equivalent to an excise tax on a new home--a tax based on the cost (either average or marginal depending on the fee schedule) of providing public services necessitated by that home. An excise tax frequently is levied when there is reason to believe that an activity engaged in by one party adversely affects the utility or welfare of another party, the effect of which is not considered in the pricing of the good or service. The fact that the activity is not priced implies the offending party has no incentive to consider the detrimental effect to the affected party (Boadway, 1979). As a result, the party causing the reduction in utility or welfare may devote an inefficient amount of resources in pursuit of the activity. Like excise taxes, impact fees are a method of diverting private resources to the public sector.

In development, certain external diseconomies are associated with growth and the production of new housing. Increased use of recreational facilities, reduction in air and water quality, and overcrowding in the schools are examples. These external diseconomies, also referred to as social costs or negative externalities, result from the provision of public services in a system experiencing increasing average costs. As mentioned previously, this indicates the system is being used beyond its point of optimum capacity. The result is the imposition of a social cost on existing users of the system. This implies that prices of new dwellings, in part responsible for the increased social costs, are artificially low. Prices do not reflect the real economic costs which in turn causes output to be greater than in situations where prices reflect true economic costs (Due & Friedlander, 1977). These social costs take the form of a reduction in the level or quality of some services. For instance, a development may cause additional traffic on the roads in the immediate vicinity, leading to increased congestion, especially during hours of heavy use. The additional time it takes existing residents to get from one place to another because of the increased congestion from new users is a measure of social cost.

The imposition of an excise tax is a traditional response to a good whose price is artificially low in terms of real economic costs and whose output is excessive. The excise tax "is designed to restrict consumption to economically justifiable levels" (Due & Friedlander, 1977, p. 367). In a somewhat similar fashion, impact fees can be considered an attempt by local government to foster economic efficiency in the housing sector. While it cannot be said with certainty that impact fees restrict consumption, it can be said that their imposition strives for an efficient solution to the problems of growth and development, provided, of course, the fees actually reflect social costs.

If an impact fee is considered to be similar to an excise tax, economic theory states that in a perfectly competitive market an excise tax levied upon the supplier of a good or service is exactly the same as a tax levied on the demander of a good or service (Varian, 1984). In other words, the price effects of the tax will be the same whether the tax is charged directly to the producer or manufacturer or whether it is assessed directly upon the consumer. Therefore, the analysis focuses on the supply side, since payment of impact fees in Florida is the responsibility of the developer, either upon issuance of the building permit or at the time the certificate of occupancy is granted.

In a purely competitive market the distributional effects of the tax will vary depending on the supply and demand elasticities of the particular market. If the supply curve is perfectly inelastic (represented by a vertical line perpendicular to the x-axis measuring quantity produced at a given price level), and demand is constant, then the tax will not affect price. Producers of the good or service will receive a lower net price. This lower net price results in either lower real income to the producer or reduction in the use of other factors of production. This situation is primarily a short-run occurrence usually encountered in markets for perishable goods, although the supply of housing is quite inelastic over short periods of time.

If the market is characterized by a perfectly elastic supply curve (represented by a horizontal line perpendicular to the y-axis measuring price demanded at a given level of output supplied), the effect of the tax would be to raise price by the full amount of the tax. (This assumes constant average costs and instantaneous adjustment of output.) Otherwise firms in the industry would leave, as they could no longer afford to compete at the higher cost level. This would continue until enough firms leave the industry to restore the average profit rate in existence prior to implementation of the tax (Due & Friedlander, 1977).

In the more likely case where the short-run housing supply curve is less than perfectly elastic (there is a degree of positive slope to the supply curve), the imposition of a tax upon a producer causes the supply curve to shift upward and to the left. The demand curve is assumed to remain the same. In other words, developers cannot immediately raise prices. In this situation the new supply curve given as $S' = (P_1 + t)$, where P_1 is the original price and t, the amount of the tax, has shifted upward to the left by the amount $P_1 + t$. That is, producers must receive a price $P_2 = P_1 + t$ per unit of housing supplied in order to supply q_2 units of a good or service, in this case housing. (See Figure 1.)

Note that at the new equilibrium point, the amount paid by consumers has risen by less than the amount of the tax t, the impact fee. That is, $P_2 < P_1 + t$. This occurs because the increase in the net price, given by the price corresponding to any point on the demand curve, has caused consumers to demand less, and the lower amount of output can be produced at a lower marginal cost.

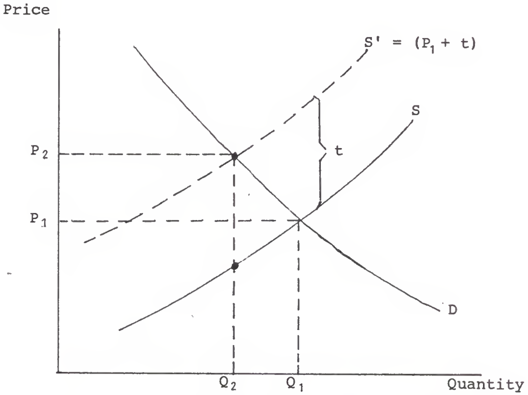

Figure 1. Short-Run Price Effect of an Excise Tax (Adapted from Varian, 1984).

However, at the new lower price which is less than average cost, developers, or homebuilders are making a negative profit. This will cause firms to exit from the market or change their technology to produce this smaller output more economically. In the long run, the supply curve of the industry is flat at the point where price equals long-run average costs; therefore, shifting the demand curve will not change the long-run equilibrium price to the producer. This means that the price to consumers of housing services will have to rise by the exact amount of the tax, or impact fee (Varian, 1984). Thus, theory states, in the short run and under the assumptions of a purely competitive market, the cost of an impact fee will be shared in some proportion by both the developer and the homebuyer. The exact proportions will, of course, depend on the particular supply and demand functions in the relevant market. In the long run, the cost of the impact fee will be completely passed forward to the homebuyer.

If the housing market were, on the other hand, a complete monopoly, the burden of the excise tax would be shared to some degree by the monopolistic builder and the homebuyer. This is because the monopolist, if he is attempting to maximize profits, will set price at that point on the average revenue curve intersected by a line extended vertically from the point of intersection of the marginal cost and marginal revenue curves, not where price equals marginal cost. Recall that in a monopoly pricing situation, the marginal revenue curve is a different and much steeper curve than the demand, or average revenue, curve. (In a competitive market the marginal and average revenue curves are the same.) An excise tax will increase the marginal cost of the monopolist.

In an attempt to balance the new marginal cost with marginal revenue, the monopolist will reduce production, or output. The level of production is given by the average revenue, or demand, curve just as it is in a nonmonopolistic market. But because the slope of the average revenue curve is less than the slope of the marginal revenue curve, price increases by an amount less than the increase in marginal cost. Note this is similar to what happens in the short run in a competitive market situation with an elastic supply and demand curve. There is a measure of cost sharing between buyer and seller. The major difference is that in a monopoly output is less than it would be in a competitive situation. Thus the efficiency with which housing is provided in a monopoly is lower than it is in a competitive framework.

A number of researchers have characterized the land development process as one of monopolistic competition (Chamberlain, 1939; Nicholas, 1982; Smith, Tschappat, & Racster, 1981). The primary difference between monopolistic competition and pure, or perfect, competition is product differentiation. Because of physical and locational differences, each parcel of real estate has a degree of monopoly advantage over competing parcels. No two parcels are exactly alike and therefore each is unique in its ability to satisfy consumer wants. Furthermore, in addition to the fact that real estate is not a homogeneous product, buyers and sellers do not have perfect knowledge of alternative, and perhaps equally satisfying choices, and the physical structure itself is not easily divisible into small units. These attributes of real estate lend additional support to the conclusion that the market in which new homes are offered for sale contains certain elements of monopoly.

However, potential homeowners have available to them alternative housing choices--other new homes, used homes, renovated dwellings, and rental properties. Even though each structure is unique there is enough similarity in the alternative properties to allow for some competition in the selection process. Likewise, a given parcel of unimproved real estate will have many similar parcels competing with it on the basis of physical or locational characteristics that consumers will find about equally satisfying. As a practical matter, "the fact that many parcels of urban land are to some extent in competition with each other for a given usage would seem to refute the idea of monopolistic control of urban land" (Smith et al., 1981, p. 135).

A key difference between pure monopoly and monopolistic competition is that the firm in monopolistic competition does not have the unique ability to satisfy the demands of the market as does the firm in a pure monopoly. In monopolistic competition the existence of profits and the fact that product differentiation leads to a downward sloping demand curve will limit the extent to which a given firm can control price. However, "due to differentiation, firms can raise their price above what the purely competitive market would dictate" (Nicholas, 1982, p. 32), but it will lose some customers. This, and the fact that excess profits will lead to the entry of new firms "with closely substitutable products" (Smith et al., p. 136), will cause profit levels to decline. Assuming demand remains constant, in the long run profits of firms in monopolistic competition will tend toward zero which is exactly the same as what happens in the purely competitive case.

The degree of cost sharing among landowner, developer or homebuilder, and homebuyer will be dependent upon the relevant elasticities in the market, or markets, under consideration. However, the primary question is: To what extent are impact fee assessments passed forward, backward, or assumed by the developer as measured by price for new homes? The answer to this question implies much about the relevant market in which the analysis is conducted. This will have practical as well as theoretical significance especially to local government officials and other policy makers who are considering adoption of, or modification to, existing impact fee ordinances. As Dowall has pointed out,

> These effects [impact fees and other land use control measures] can have considerable impacts on the operation of housing markets. The degree or magnitude of impact depends on the demand for land and housing and on the way that local policy affects the supply of residential land and housing. However, most local planning agencies do not have accurate measures of land supply and residential development potential. (1981, p. 146)

Previous work which attempted to determine price elasticities of supply and demand for housing provides some indication as to the price effects of an impact fee in markets characterized by different supply and demand functions (Clawson, 1971; Diamond, 1984; Lee, 1964; Muth, 1969; Witte, 1975). These functions are represented by curves which are elastic, inelastic, or which exhibit a slope, in absolute terms, of greater than zero but less than infinity.

In a market characterized by highly elastic housing supply and demand curves (curves have the same absolute slope), any fee or development charge will likely be shared approximately equally by developer and homebuyer. The most significant short-run effect in this situation might be a reduction in the overall level of housing provided, unless builders can adjust their factor costs downward to offset the amount of the fee (Weitz, 1985). In the long run the outcome is less certain. If demand remains strong in the area, prices of existing properties and new homes in other (substitutable) areas may be bid up. In this case the price of the house will likely include most of the impact fee, provided landowners do not increase prices. Note that if demand rises, and the cost of land remains stable relative to competing parcels in substitutable markets, a portion of the development fee is capitalized into lower land values and, passed backward to the landowner through lower bid prices.

A similar situation exists in markets characterized by highly inelastic housing supply and demand curves. Once again this assumes the curves have the same absolute slope. The analysis of the short-run and long-run effects would be the same with the only major difference being that any decline in actual housing production would be less. This is due to the fact that the slope of the supply and demand curves is greater, and consequently, less elastic than in the previous case.

In a housing market typified by a rather inelastic demand and highly elastic supply, a market found to exist in the empirical work of Muth (1969), most, if not all, the cost of an impact fee is passed along to the buyer. This may be mitigated somewhat if there are substitutable markets from which prospective buyers may choose. If one market is constrained by development fees and the other is not, developers may find homebuyers moving to a different market where prices are lower for comparable homes. The builder has the choice of either moving to an area where there are no fees or remaining in the current location and competing by paying some portion of the impact fee or reducing the amount spent on other factor inputs, land or labor. As development fees become more pervasive and opportunities to purchase housing at a price differential based on the absence of impact fees decline, the elasticity of demand will likewise decline. This will increase the likelihood that most of an impact fee will be passed forward to the buyer.

Production in the latter case also may decline but probably much less than it will in a market with relatively high elasticity of demand. The value of developable land, therefore, will also decline. Again, the extent and timing of the decline will be less severe than in the first case. However, any decline in production in the latter case will probably be greater than that which will occur in a market with highly inelastic supply and demand, the opposite of the first case.

Another reason to assume demand for new, single-family housing may be relatively inelastic, particularly in states like Florida, is the nature of the state's population growth. In markets characterized by strong demand for new housing and a lag in the provision of new homes, price increases may not significantly affect quantity demanded. This is especially true of certain areas of Florida where there is steady net inmigration. Existing housing can be expected to accommodate some, but certainly not all, of the new residents moving to the state.

The ease of entry and exit into the home building industry has been cited by Muth (1969) as one of the reasons for assuming the industry is characterized by a highly elastic supply curve. Landis (1986) also implied that ease of entry and exit make for a highly elastic supply function.

Theory states that firms enter or exit an industry depending on the profit opportunities available. As costs increase, marginal builders will exit the industry and remaining builders will absorb costs through lower profits. Eventually production will start to decline and additional builders will begin to leave the industry. An obvious result of this will be a decline in the annual supply of new homes or a decrease in the rate at which new homes are provided.

Table 1 illustrates the movement of residential building contractors into and out of the construction industry for recent years. Also included are the number of single-family housing starts which reflect changes in supply for the same time period.

There appears to be a lagged relationship of approximately 15 to 18 months between the number of starts and the increase or decrease in the number of residential building contractors. In the building industry, it is well known that a lag exists given the time it takes to finish construction of a new home as well as the fact that builders do not always begin construction as soon as a permit is issued. Furthermore, it may take a builder a year or more to build all the homes for which he has permits.

Note the increase in the number of builders in 1984, when the decrease in housing starts between 1981 and 1982 would have led one to expect a further reduction in builders. This may have been due, however, to the decline in interest rates, which made new homes more affordable than they had been since the late 1970s. Another explanation may be that builders shifted into construction of multi-family housing.

Table 1

Residential Builders and Housing Starts--Florida.

Year	Builders[a]	% Change	Starts	% Change
1978	5,781	--	97,500	--
1979	6,299	8.96	112,400	15.28
1980	6,534	3.73	87,200	-22.42
1981	6,928	6.03	66,200	-24.08
1982	6,844	-1.21	56,600	-14.50
1983	6,647	-2.89	101,900	80.00
1984	7,497	12.79	99,400	-2.40

Note. The data in column 2 are adapted from Shoemyen (1985). The data in column 4 are adapted from The Florida Outlook, Vol. 9. No. 4.

[a] As of March of each year.

In 1983 and 1984 there were 6,647 and 7,497 licensed residential building contractors respectively, and just over 100,000 and 99,000 single-family housing starts. The data suggest that many builders are each building a relatively small number of homes on an annual basis, although it is highly probable that in some areas a few large builders are responsible for a large percentage of the finished homes. This can be considered further evidence supporting the contention of Muth (1969) and

Landis (1986) that the industry is characterized by ease of entry for new firms. Staying in business is another matter. Builders can exit the industry even more easily than they enter.

CHAPTER III
MODEL DEVELOPMENT

As previously stated, the primary purpose of this research is to examine the effect of impact fees on the price of new, detached, single-family housing. A secondary objective is to test whether impact fees levied in one or more cities in a housing market affect new home prices in other cities in that market which do not have impact fees. The study investigates the price effect of impact fees on the population of new home prices in five Florida cities from 1970-1984.

Cities were selected from within the Pinellas County metropolitan area where, over the period of study, two of the cities implemented impact fees while the others did not. This allows comparison of new home prices before and after the imposition of impact fees to determine whether impact fees contributed significantly to higher price, either directly or indirectly. The threshold level for determining at what point in time a city had impact fees in an amount sufficient for inclusion in this study is $1,000. Any subsequent increase or decrease in the fee schedule is ignored.

Assumptions

Following Muth (1960), Olsen (1969), and Zorn, Hansen, and Schwartz (1986), housing is defined as a distinct, homogeneous, bundle of services comprised of measurable physical, site, and locational characteristics. Housing services are provided in a perfectly competitive market with all housing possessing common attributes, although differing in quantity of attributes per unit of housing.

The assumption of perfect competition, while not strictly true, is convenient for modeling purposes (Kau & Sirmans, 1985) and, in the long run, yields results which are similar to those obtained in a market characterized by monopolistic competition (Smith et al., 1981).

Zorn et al. (1986) contend that the assumption of perfect competition

> is sufficient to equate the per-unit price of housing services (within a single [city]) across all levels of housing quality. Since the level of public services within a [city] is constant, any variation in price between housing units is due to variations in the quantity of . . . attributes of the units. (p. 47)

For purposes of this study each location within a city is assumed equally desirable from the standpoint of total access to all other destinations within that city. While one location may be preferable to a homebuyer from the standpoint of access to place of employment, another location might be preferable because of its ready access to recreational opportunities and shopping centers. Each location offers advantages and disadvantages regarding linkages to other frequently accessed destinations. But, in the aggregate, no one location is more desirable than another.

Public services are assumed equal and of similar quality in each city, although not necessarily equal or of similar quality across all cities--a situation that may be in part responsible for the imposition of an impact fee. Because of these assumptions, a person's choice of housing purchase in a particular city will be determined primarily by the physical characteristics of the structure and the neighborhood.

Macroeconomic variables such as mortgage interest rates, changes in the money supply, the level of unemployment, and others are considered to affect new home prices similarly across all cities. Assuming housing is priced as a bundle of individual and measurable attributes (Rosen, 1974), the predicted price will be obtained by using the hedonic method of regressing price on the individual characteristics comprising the unit.

Functional Form

Hedonic theory is deficient with respect to the a priori specification of the "best" functional form of the hedonic model to be used in analysis of a particular market (Halvorsen & Pollakowski, 1981). It is possible that several functional forms perform equally well in predicting price when fitted to a set of data. As Butler

(1982) noted, "researchers who have compared alternative functional forms for hedonic indexes of housing have found little basis for choosing one form over another" (p. 97). While logarithmic and semilogarithmic models frequently have been found to be the most satisfactory with respect to representing the functional relationship between prices and attributes, linear forms also have been found to yield satisfactory results. The semilogarithmic model has the advantage of allowing for variation in the dollar value of a characteristic so that the price of one component depends in part on other variables in the home (Follain & Malpezzi, 1980). Also, this form gives values of the dependent variable in percentage terms rather than absolute dollars, which facilitates comprehension and comparison of time and intercity price differentials. Therefore, as a first approximation to the correct functional form of the hedonic model, a linear relationship is hypothesized with a log transformed dependent variable, a variation of the semilogarithmic model.

Hedonic Estimate

The estimate of the true hedonic model is derived from work done by Zorn, Hansen, and Schwartz (1986). This estimate is limited to a small subset of the potential independent variables because, as Butler (1982) demonstrates, the use of a restricted model as opposed to a more complete model only results in a small decrease in the explanatory and predictive powers of the equation. A limited model is attractive also from the standpoint of efficiency, interpretability, and reduced cost.

In addition to independent variables for total living area, TOTLIV, and lot size, SQFTLOT, used by Zorn et al., the model includes a price index comprised of dummy variables indicating year of sale to capture the impact of time on housing price variability (Mark & Goldberg, 1984). Each dummy variable is assigned a value of either zero or one depending on whether or not the sale occurred in that year. It is anticipated the price index, with 1984 as the base year, will result in negative adjustments to price decreasing in magnitude from 1970 to zero in 1984. Also, if the index performs as expected, it should pick up the large increase in housing prices from 1972-1974 and again in the 1978-1980 time period.

Given that the Pinellas County metropolitan area is the relevant housing market (Follain & Malpezzi, 1980; Mills, 1967; Muth, 1969), and further that cities within the metropolitan area are local and diverse, dummy variables also are included in the model to measure intercity locational differences. A priori, there is no way to determine the likely sign or magnitude of these parameter estimates. However, one would anticipate, ceteris paribus, that housing in a city considered more desirable than that in the base city would warrant a price premium from homebuyers desiring to reside in the preferred location. A price premium, if significant, will result in a positive parameter estimate for the independent variable denoting the city of preference.

Before proceeding it must be noted that all estimates of the hedonic function for housing are to some extent misspecifications of the true relationship between price and housing characteristics and therefore subject to some degree of specification bias (Butler, 1982). However, it is impossible to include all characteristics relevant to the determination of price because of cost and other data constraints. Further, even if cost and data constraints are not of concern, "the intrinsic clustering of characteristic combinations into a relatively small number of configurations leads to considerable collinearity in estimates employing a generous selection of relevant variables" (Butler, p. 97). To deal with the problem of collinearity certain variables are necessarily excluded from the model. The elimination of relevant variables will, of course, cause the model to be subject to some degree of misspecification. But, as Butler found, "the practical impact of these [misspecification] biases is small and even the severely restricted [models] . . . appear to suffer only limited coefficient bias, with negligible impact on the explanatory and predictive powers of the equation" (p. 106).

The database includes information on selected physical characteristics of each property, such as total conditioned space, wall type, and lot size, but does not include any data relating directly to neighborhood characteristics. Therefore, a proxy variable is developed as an indicator of the relative desirability of a neighborhood. The variable, SQFTCOS in the model, is calculated by multiplying the ratio of land value to total assessed value by the original sale price and then dividing by the number of land units, or square feet of the lot. This results in a measure of the square foot cost of the land when the home was originally built.

The rationale is that properties in more desirable neighborhoods will have higher assessed values than comparable properties in less desirable neighborhoods and therefore a higher square foot cost. Also, the assumption is made that the ratio of assessed land value to total assessed value is constant over time. Additionally, because local property appraisers may be one, two, or more years behind in the assessment of a certain portion of properties in a county, there is no way of knowing for certain in what year the assessed value is determined. Not knowing this prevents the use of assessed value of land, deflated by an appropriate price index, as a variable to measure the relative desirability of a neighborhood.

This study contrasts with that of Zorn et al. by assuming there is some degree of dependency among cities within a housing market. Because of this potential dependency, an impact fee levied in one city may affect the price of a new home in another city, one that does not have impact fees. Therefore, an independent variable, #CITY, is included to measure the effect of impact fees levied in other cities. This variable records the total number of cities imposing impact fees excluding the city in which the sale takes place. As more cities enact impact fee ordinances, it is anticipated that the price of new, single-family dwellings in nonimpact fee cities will rise as demand for comparable housing shifts away from cities subject to impact fees.

The model includes several interactive, explanatory variables, FEECIT1, FEECIT2, FEECIT3, and FEECIT4, which measure the city-specific effect of impact fees. This is in addition to the independent variable FEE, the regression coefficient of which measures the unspecified city effect of impact fees across all cities in the study. The FEE variable is a dummy variable that has a value of one if a city has impact fees totaling at least \$1,000, and zero if there are no fees or fees totaling less than \$1,000 per single-family dwelling. With the inclusion of the interactive terms, the change in the mean response is given by the estimate for $b_4 + b_m FEECIT_n$ (b_m is the regression coefficient corresponding to the interactive, explanatory variable FEECITn, where n denotes the city in which the sale occurred. It is expected that the regression coefficient of b_m will be zero when sales occur in a city with no impact fee.

Diagnostics

Prior to testing the model, the distribution of the data by city on an annual basis will be examined to determine whether any apparent data anomalies are evident. Scatter diagrams, or plots, also will be examined, before and after fitting the model, to determine whether any spurious points, such as outliers, exist. However, as noted by Belsley, Kuh, and Welsch (1980), "diagnostics of this type [while beneficial] cannot detect multivariate discrepant observations, nor can they tell us in what ways such data influence the estimated model" (p. 7).

Outliers are extreme observations that, in a residual plot, lie far beyond the scatter of the remaining residuals, perhaps three or more standard deviations from zero. The residual, of course, is the difference between the observed value and the value predicted by the regression model. An outlier is cause for concern because, "under the method of least squares, a fitted line is pulled disproportionately toward an outlying observation because the sum of the squared deviations [residuals] is minimized" (Neter & Wasserman, 1974, p. 107).

According to Atkinson (1985), an outlier can occur for a number of different reasons:

1). There may be gross errors in either the response or explanatory variables due to errors in the measurement or recording of the data.

2). The estimated model may be inadequate to describe the systematic structure of the data, as, for example, when the outlier occurs because of interaction with another explanatory variable omitted from the model.

3). The data may be better analyzed in another scale, such as logarithmic.

4). Least squares may not be the best method of fitting the model.

After the regression has been performed, the method of studentized residuals will be employed to determine whether there are any extreme data points imposing a disproportionate influence on the estimated model. This procedure is used to determine whether deletion of the *i*th observation from the data set has a marked effect on prediction. More specifically, does the actual response variable agree with the predicted response variable which results when the *i*th observation is not used in the calculation of the regression (Atkinson, 1985)? This method is particularly useful for identifying influential observations which, because of a high degree of leverage, have relatively small residuals. The small residual is due to the effect of high leverage which forces the fitted model close to the observed value of the response variable (Atkinson, 1985). Under these circumstances it is unlikely that an examination of the normal residual plots would reveal the influential observations.

The computer printout of studentized residuals will be examined to identify any observations with residuals lying +/- two or more standard deviations from zero. It will then be determined whether these outliers are caused by error. If so, these observations will be deleted from the dataset. Outliers determined to represent valid observations will be retained in the dataset.

Another problem and, according to Intriligator (1978), "perhaps the most ubiquitous, significant, and difficult to deal with" in studies such as this, is the problem of multicollinearity, or simply collinearity (p. 152). Collinearity exists when the independent variables are correlated among themselves, or correlated with other independent variables that are related to the dependent variable, but not included in the model. As a result, the effect of the independent variables on the dependent variable cannot be accurately measured by the regression coefficients. The inability to identify unique values for the explanatory variables would not be of critical concern if this study were being conducted solely for the purpose of developing a model with which to predict new home sale prices. However, the primary purpose of this research is to examine the specific effect of impact fees on new home sale prices, the dependent variable in the model. Therefore, it is critical that any collinearity existing in the model be recognized and dealt with in a statistically appropriate manner.

The presence of multicollinearity is indicated by contradictory signs of regression coefficients from those hypothesized and low t ratios coupled with a high F statistic. The low t ratios indicate few, if any, of the regression coefficients are significantly different from zero, while the high F statistic implies that the hypothesis that all coefficients are zero should be rejected (Intriligator, 1978). Further, an examination of the matrix of correlation coefficients for high values will indicate the presence of a linear relationship between two variables. However, low values do not signify completely the absence of a collinearity problem, as it is possible for a linear relationship to exist among several variables.

Several remedial measures exist for dealing with the problem of collinearity: First, the sample can be augmented with additional data in hopes of breaking the pattern of multicollinearity (Neter & Wasserman, 1974). Second, the model can be scaled down or simplified to represent better the data available (Intriligator). The third approach is to recognize the problem and not try to change either the data or the model (Intriligator). As stated previously, however, the purpose of this study is to isolate the effect of a particular independent variable. Therefore, the presence of any collinearity must be addressed and cannot be ignored.[1]

The Model

The original model is given as follows:

$$\begin{aligned}\text{Log NHSP} = {} & b_0 + b_1\text{TOTLIV} + b_2\text{SQFTCOS} + b_3\text{SQFTLOT} + b_4\text{FEE} \\ & + b_5\text{\#CITY} + b_6\text{YR1} + b_7\text{YR2} + b_8\text{YR3} + b_9\text{YR4} + \\ & b_{10}\text{YR5} + b_{11}\text{YR6} + b_{12}\text{YR7} + b_{13}\text{YR8} + b_{14}\text{YR9} + \\ & b_{15}\text{YR10} + b_{16}\text{YR11} + b_{17}\text{YR12} + b_{19}\text{YR13} + \\ & b_{20}\text{YR14} + b_{21}\text{CIT1} + b_{22}\text{CIT2} + b_{23}\text{CIT3} + \\ & b_{24}\text{CIT4} + b_{25}\text{FEECIT1} + b_{26}\text{FEECIT2} + \\ & b_{27}\text{FEECIT3} + b_{28}\text{FEECIT4} + \text{error}_i\end{aligned}$$

where

Log NHSP = Natural log of new home sale price;
$b_{(0...28)}$ = Unknown hedonic coefficients to be estimated;
TOTLIV = Conditioned space of dwelling in square feet;
SQFTCOS = Square foot cost of land;
SQFTLOT = Lot size in square feet;
FEE = Dummy variable indicating presence or absence of impact fees totaling at least $1,000 per single-family dwelling;
#CITY = Number of cities in study having impact fees;

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[1] Subsequent testing of the model revealed the presence of collinearity. An alternative model and methodology were required to solve the problem. This process is described in detail in Chpater IV.

YR1...YR14 = Price index comprised of dummy variables indicating year of sale; an endogenous adjustment for inflation with base year 1984;

CIT1..CIT4 = Dummy variables indicating city in which sale occurred;

FEECIT1.. FEECIT4 = Interactive terms measuring city-specific effect of impact fees;

e_i = error E [e] = 0, Var [e] = constant, E $[e_i, e_j] = 0$.

Data

Magnetic data tapes containing information on all parcels of real property in Pinellas County were obtained from the Florida Department of Revenue in Tallahassee.

Criteria for selecting cities in the study are as follows:

1. Other than the imposition of impact fees, cities could not implement policies that would cause an increase in housing costs--a water or sewer moratorium, for instance.

2. Cities had to be located within the same housing market.

3. Population demographics and physical characteristics of the housing stock in the cities had to be similar.

As this study uses a large, secondary data source, it was inevitable that data problems would exist. By necessity, a considerable amount of time was expended to ascertain where problems were most likely to occur and how best to deal with them. A determination also had to be made regarding the most efficient manner in which to subset the desired data.

At this point it was impossible to know the existence or extent of any data problems. Therefore, the discussion of database examination, removal of problematic or erroneous data, and subsequent subsetting is deferred to Chapter IV of this dissertation.

CHAPTER IV
RESULTS

Database

Prior to testing the model, two, 2,400 foot magnetic data tapes (6,250 bytes per inch) were entered into the tape library of the Northeast Regional Data Center at the University of Florida. These tapes, provided by the Florida Department of Revenue's Division of Information Systems and Services, were subsequently tested to determine whether any portions of the tapes were defective and mapped to ascertain the extent and format of the information available. Containing the county property appraiser data from the name, address, and legal file on every parcel of real property in Pinellas County, Florida, these tapes provided a total of more than three hundred thousand individual records (logical record length = 450 characters), each with 51 items of information.

A partial tape dump, which allowed for manual inspection of each volume, was done to determine the most efficient way to extract data on new home sales. Some of the information contained in each record includes: the tax identification number and legal description of the parcel, the name and address of the owner, total assessed value before and after property tax exemptions, assessed value of the land, and square footage of the improvement and lot. In addition, each record contains the year in which the dwelling was built, its sale price and the date of the transaction for each of the two most recent sales, if a second sale exists. Further, each record contains a code indicating the land use classification, a taxing authority code which identifies the city in which the property is located, and a code identifying whether the property was vacant or improved at the time of the first and/or second most recent sale.

A program was written to identify all records where the year built matches either of the two most recent sale dates and the land use code is that of a detached, single-family dwelling in one of the five cities selected for the study. In this manner, total, annual new home sales were determined for each city.

The resultant dataset contains 13,704 records of information on new home sales in five cities from 1970 through 1984 (the most recent year for which data were available). Note this dataset contains only those observations on single-family dwellings experiencing fewer than three exchanges of ownership during the period of study. There is no way to determine from the database the original sale price of any property sold more than twice.

The five cities meeting the specified criteria and selected for this research were Dunedin, Clearwater, St. Petersburg, Safety Harbor, and Palm Harbor. Dunedin was the first city to implement impact fees totaling more than $1,000. Effective June 3, 1974, Dunedin assessed impact fees for sewer and water in the amount of $1,150 per single-family unit. Clearwater had a development impact fee of $250 per unit effective May 7, 1973, and a water impact fee of $275 per unit effective September 18, 1980. It was not until July 7, 1983, however, when the city assessed a recreation impact fee of $200 per unit and an open space impact fee of four percent of assessed land value, that Clearwater had impact fees of more than $1,000 per unit--the threshold level for determining the presence or absence of impact fees. With a mean assessed land value of $25,792 in 1983, total impact fees for a new, single-family residence were approximately $1,750 per unit. St. Petersburg, Safety Harbor, and Palm Harbor had no impact fees or had fees totaling less than $1,000 per single-family unit.

Data Constraints

As predicted, the database was found to have a wide range of values for each item of information included in a record. For instance, observations in the database range in sale price from $0.00 to $6,560,000 per unit. Observations range in size of dwelling from 639 to 6,156 square feet. A frequency distribution of annual home sales by city was printed in order to determine the general distribution of the data. Scatter plots also were examined to determine whether any obvious data anomalies such as gaps or outliers exist. Extreme values, while some are legitimate, often are due to errors in data entry or represent atypical transactions. Atypical sales may be the result of nonarm's length transactions, such as between family members, or the result of sales between individuals, one or both of whom has erroneous or inadequate information regarding market value. The most common data entry errors involve a zero or missing value for sale price and number of land units (square feet) of the lot.

Observations that contain data entry errors or that represent an atypical sale were eliminated at this stage of the project. To accomplish this, a determination had to be made as to the upper and lower bounds of sale price to be included in the study. It was decided to impose an upper bound constraint of $175,000 upon the data for two reasons. One, less than 4 percent (3.52) of the total number of observations for the 15 years had a total sale price greater than $175,000. Second, it is quite possible that sales in excess of $175,000 represent properties located on beachfront, either the Gulf of Mexico or Tampa Bay. The database contains no information that allows this possibility to be readily confirmed. Regardless, sales of new, single-family, waterfront property are a very small percentage of annual, new housing supply and certainly not the type of housing where home purchases are likely to be sensitive to impact fees.

Another problem that exists within the database is that many observations coded as being an improved, single-family property have a corresponding sale price which represents the price originally paid for the lot. These are properties purchased by an individual who later constructed a dwelling on the lot and has since occupied that home. Therefore, the dollar value listed as the sale price of the property as improved is actually the price paid for the lot. In order to deal with this situation, a three-level screening process was imposed to delete observations that represented land sales. From 1970 to 1974, any observations with a sale price less than $25,001 were deleted. From 1975 to 1979, any observations with a sale price less than $30,001 were deleted. From 1980 to 1984, any observations with a sale price less than $35,001 were deleted. These threshold levels were determined after reviewing data on the mean sale price of new, single-family homes in Standard Metropolitan Statistical Areas and Metropolitan Statistical Areas published by the U. S. Department of Commerce and the Department of Housing and Urban Development from 1970 to 1984. This treatment of the data also has the advantage of eliminating observations with a zero or missing value for the sale price.

It must be recognized, however, that even with this screening process it was unavoidable that some land sales remained in the database and that some bona fide home sales were lost. With respect to the land sales it was expected that when the studentized residual statistics were examined, these observations could be identified and deleted. The loss of some home sales in the lower price ranges was inevitable.

One additional criterion was imposed to screen the data prior to testing of the model. Any observation with a lot measurement greater than two acres was deleted. This was done to eliminate observations coded as detached, single-family residential which had lot sizes much larger than the mean lot size (10,491 square feet) for all such similar homes, or that were actually nonresidential properties bearing an incorrect land use code. For instance one property, identified by land use code as a single-family dwelling, had a land measurement of more than sixty-three million square feet, or about 1,450 acres. While it was not possible to verify the validity of this observation, the fact is, data such as this are highly suspect and typify problems encountered in using large, secondary data sources.

Following elimination of data considered to be in error or atypical of the majority of data describing single-family dwellings, 10,294 observations remain in the dataset with which to test the proposed model. Table 2 summarizes the annual number of new home sales by city.

Table 2

Annual Sales of Single-Family Dwellings by City.

	Dunedin	Clearwater	St. Petersburg	Safety Harbor	Palm Harbor	
1970	5	82	63	0	0	150
1971	31	97	92	0	0	220
1972	109	144	140	1	0	394
1973	89	201	130	23	39	482
1974	129	159	119	15	18	440
1975	51	150	71	0	25	297
1976	32	157	61	10	83	343
1977	114	239	56	75	112	596
1978	96	304	136	120	338	994
1979	139	487	163	105	432	1,326
1980	103	240	106	182	546	1,177
1981	69	192	100	199	454	1,014
1982	36	107	25	158	341	667
1983	48	139	65	176	602	1,030
1984	39	84	82	219	740	1,164
Total:	1,090	2,782	1,409	1,283	3,730	10,294

Initial Testing

The Statistical Analysis System (SAS) was used to perform the analyses of the data. Initial testing revealed that collinearity among the city dummy variables and the impact fee variable was a problem. This was evidenced by high global model F values with low individual t statistics for the offending variables and regression coefficients with signs opposite of those expected.

The problem was exacerbated with the inclusion of the variable recording the number of cities imposing an impact fee and the interaction terms. Furthermore, instability of the regression coefficient for the fee variable was noted, depending on whether the model was run with or without the city dummy variables. The parameter estimate for the impact fee variable ranged from negative and significant to positive and significant at the alpha = .05 level.

The usual methods for addressing the problem of collinearity--dropping one or more of the offending regressor variables, adding more data in hopes of breaking the pattern of collinearity, or changing the model--were considered. Given the objective of this research, dropping the fee variable was out of the question. Deleting the city dummy variables may have solved one problem, but would likely have resulted in another. With the elimination of the city dummy variables it would not be possible to determine whether the impact fee variable was indeed measuring the effect of impact fees or whether it was picking up the locational effect of the missing city dummy variable. The confounding of the city and impact fee effects would make it impossible to infer anything about the price effects of either variable. Adding more data likely would not have made much difference considering the fact that over 10,000 observations were already included in the database.

An attempt was made to fit the model to individual city data eliminating the interaction terms, the city dummy variables, and the variable denoting the number of other cities imposing impact fees. However, this approach led to problems with singular matrices[1] caused by the linear dependency resulting from the use of a dummy variable to indicate the point in time at which impact fees became effective and the dummy variables comprising the price index. In addition to biased estimators, the problem remained of how to account for location in this reduced model. Because of the collinearity problems and the difficulty encountered in fitting an individual city model, it was clear that a different approach to the problem was mandated.

Revised Model

It was obvious that to achieve the primary objective of this research the model would need modification. In order to focus fully on the issue of fee incidence and to ascertain the extent impact fees are capitalized into price, the secondary objective of determining the indirect effects of impact fees upon house prices in cities without fees was abandoned.

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[1] A singular matrix occurs when a row or column of the matrix can be written as a linear function of some other row or column. The determinant of a singular matrix is zero, thus no inverse of the variance-covarinace matrix exists.

The resultant model incorporates many elements of the original model with one notable exception. The new model eliminates the regressor variable denoting the presence or absence of an impact fee, a concession to the collinearity problem. This change in the model dictated a change in the methodology as well. Instead of attempting to estimate the effect of impact fees directly within the hedonic framework, the model is fitted to the data without impact fees and the results used to predict sale price of those observations subject to an impact fee. It was anticipated that a consistent pattern would result where the ratio of actual sale price to predicted sale price would be, on average:

1. less than one if predicted price is greater than actual sale price,

2. equal to one if predicted price is equal to actual sale price, and

3. greater than one if predicted sale price is less than actual sale price.

A mean ratio less than one would imply that impact fees are not contributing to higher prices, while a mean ratio equal to one would indicate the model is incapable of discerning any difference between the two. A mean ratio greater than one would lead to the belief that impact fees are contributing to higher new home sale prices. From this it can be inferred whether impact fees are contributing to higher sale prices in cities with fees compared to those without fees.

The assumptions underlying the revised model are essentially the same as those for the original model. The one exception is that the revised model assumes that any relative price effects of intercity locational differences remain constant over the period of study. This assumption makes it possible to infer that any difference between predicted and actual sale price in cities with fees is due to the fee and not to changes in the relative attractiveness of either of the two cities compared to the others in the study subsequent to imposition of impact fees.

The revised model is given as follows:

$$\begin{aligned} \text{Log NHSP} = {} & b_0 + b_1\text{TOTLIV} + b_2\text{SQFTCOS} + b_3\text{SQFTLOT} + b_4\text{YR1} \\ & + b_5\text{YR2} + b_6\text{YR3} + b_7\text{YR4} + b_8\text{YR5} + b_9\text{YR6} + \\ & b_{10}\text{YR7} + b_{11}\text{YR8} + b_{12}\text{YR9} + b_{13}\text{YR10} + b_{14}\text{YR11} \\ & + b_{15}\text{YR12} + b_{16}\text{YR13} + b_{17}\text{YR14} + b_{18}\text{CIT1} + \\ & b_{19}\text{CIT2} + b_{20}\text{CIT3} + b_{21}\text{CIT4} + \text{error}_i \end{aligned}$$

where

Log NHSP = Natural log of new home sale price;

$(b_0 \ldots b_{21})$ = Unknown hedonic coefficients to be estimated;

TOTLIV = Conditioned living space in square feet;

SQFTCOS = Land cost per square foot;

SQFTLOT = Lot size in square feet;

YR1...YR14 = Price index comprised of dummy variables indicating year of sale; an endogenous adjustment for inflation with base year 1984;

CIT1..CIT4 = Dummy variables indicating sale location. CIT1=Dunedin (DN), CIT2=Clearwater (CW), CIT3=St. Petersburg (SP), and CIT4=Safety Harbor (HR). City with largest number of new home sales (Palm Harbor) is the base city;

$error_i$ = Error term with usual assumptions.

The interaction terms, FEECIT1, FEECIT2, FEECIT3, and FEECIT4, also are eliminated from the model as the revised methodology allows the difference between predicted sale price and actual sale price to be computed on an individual city basis. The other elements in the model remain the same.

Fitting the Model

The analysis was accomplished in three steps. First, the model was fitted to the data not subject to an impact fee--a total of 9,395 observations--using the regression procedure in SAS which fits least squares estimates to linear models. For Dunedin, this includes sales through the year 1975. This represents a lag of approximately 18 months from the time impact fees first became effective in that city. The lag is incorporated into the model in the belief that builders in anticipation of the $1,150 of increased costs would apply for building permits prior to the effective date at which fees became mandatory. Thus, a substantial number of new homes not subject to an impact fee could be built and sold after the implementation of the impact fee ordinance. Data on building permits issued in Dunedin from 1974 to 1975 confirms this phenomenon. Building permit activity for single-family dwellings declined from 226 in 1974 to 72 in 1975 (Thompson, 1975, 1976).[2,3] Thus, it is likely that many of the new home sales occurring in 1975 were those for which a permit was issued prior to June 3, 1974, and no fee was paid.

The situation in Clearwater was somewhat different. While a development impact fee of $250 per unit had been in effect since May 7, 1973, and a water impact fee of

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[2] In the year prior to the implementation of impact fees in Dunedin, the total number of homes built and sold increased from 89 in 1973 to 129 in 1974 while the total number of homes built and sold in the other four cities declined from 529 in 1973 to 395 in 1974. A chi-square test was conducted to determine whether there was any statistical association between the number of homes built and sold in Dunedin and the number of homes built and sold in the other cities in 1973 and 1974. The value obtained from the chi-square test (19.165) leads to the rejection of the hypothesis of independence at the .001 level (10.828). This would imply that builders in Dunedin were active in anticipation of higher costs due to impact fees at a time when builders in other cities were slowing down.

[3] The model also was fitted to the data using 1974 as the benchmark year, with all homes built in Dunedin after 1974 considered to have been subject to impact fees. In effect, this allowed for a 6 month lag from the time the fee ordinance was enacted (June 3, 1974) to the date when builders would have had to pay fees in order to be issued a building permit. The results of the subsequent regressions yielded a higher standard error when using the 6 month lag. Later estimates of the mean ratio of actual sale price to predicted sale price were greater than one and statistically significant in both cases.

$275 per unit since September 18, 1980, it was not until July 7, 1983, that the total of all impact fees amounted to more than $1,000 per unit. The increment in total fees charged effective July 7, 1983, was about $1,225 to approximately $1,750 per single-family unit. This number is not exact because a portion of the fee is calculated as a percentage of assessed land value and thus will vary from one dwelling to another. Note this increase was only $75 more than that experienced in Dunedin nine years earlier when that city first imposed impact fees.

The model assumes that new home sales in Clearwater prior to 1983 are not subject to a fee and that all sales after 1982 are subject to fees. An examination of building permit activity in Clearwater showed an increase in building permits for single-family dwellings from 270 in 1982 to 352 in 1983 (Shoemyen, 1984). Unless all the permits in 1983 were issued prior to July 7, it does not appear that builders engaged in any stockpiling of permits in order to avoid the increased costs associated with the new fees. It is quite probable that the increase in permit activity was a direct result of declining mortgage interest rates from record levels in 1982, overpowering the incremental effect of increased impact fees. The total number of homes in the database subject to an impact fee in the two cities is 899; 701 in Dunedin and 198 in Clearwater.

The first regression yielded a coefficient of multiple determination (R^2) of .8858, an F value of 3,460.45 (significant at .0001), and a root mean square of .1352. As a first approximation, almost 89 percent of the variation in log price is explained by the fitted equation. The F value, which far exceeds the critical value of about 2.27 for the model degrees of freedom, implies that the natural logarithm of new home sale price is indeed related to the selected regressor variables in the model. Table 3 contains a summary of the parameter estimates, standard errors, and t statistics for the independent variables in the model resulting from the first regression run on the data not subject to an impact fee.

The root mean square, a measure of the variability in predicted sale price unaccounted for by the model, estimates the standard deviation of the predicted value for the dependent variable. Thus, for any given level of the independent variables, the variability associated with the predicted value, as measured by the standard deviation, is approximately 15 percent, calculated as the antilog of the root mean square minus one [(antilog .1352) - 1] = .1448. This measure of variability, while large for any single observation, is relatively unimportant considering the ultimate goal of this research. The important measure of variability is that associated with the average effect of impact fees over a large number of sales (the standard error of the mean) rather than the effect of impact fees on the sale price of any single dwelling.

Table 3

Regression Results--First Run.

Independent Variable	Parameter Estimate	Standard Error	T-statistic
INTERCEPT	10.03791552	0.00879917	1140.78
TOTLIV	0.00039044	0.00000334	116.86
SQFTCOS	0.17825859	0.00204034	87.37
SQFTLOT	0.00000862	0.00000020	42.83
YR1	-0.67625859	0.01309524	- 51.64
YR2	-0.62134848	0.01146055	- 54.22
YR3	-0.56403654	0.00985560	- 57.23
YR4	-0.47765035	0.00898635	- 53.26
YR5	-0.42100313	0.00920877	- 45.72
YR6	-0.36256507	0.01006868	- 36.01
YR7	-0.37025711	0.00954350	- 38.80
YR8	-0.33556947	0.00831632	- 40.35
YR9	-0.26734149	0.00684928	- 39.03
YR10	-0.16927568	0.00632511	- 26.76
YR11	-0.09816223	0.00608287	- 16.14
YR12	-0.06950338	0.00625967	- 11.10
YR13	-0.06870581	0.00689520	- 9.96
YR14	-0.06413526	0.00632001	- 10.15
DN	-0.05816044	0.00838177	- 6.94
SP	-0.11694516	0.00487914	- 23.97
CW	-0.02570515	0.00416283	- 6.18
HR	0.01268087	0.00469809	2.70

The price index behaves as anticipated with large negative parameter estimates in the early years declining to about a -6.21 percent discrepancy between 1983 sale prices and base year 1984 sale prices. As expected, prices exhibit a considerable increase in the early 1970s and again from 1978 to 1980. Each estimate is significant at the alpha = .05 level. Parameter estimates for Dunedin and St. Petersburg indicate sale prices were 5.65 percent and 11.04 percent less, respectively, than sale prices in the base city of Palm Harbor. Sale prices in Clearwater were 2.54 percent less than those in Palm Harbor while sale prices in Safety Harbor were 1.28 percent greater than those in the base city. A 95 percent confidence interval constructed for the difference between the mean sale price in Dunedin and Palm Harbor is approximately -7.22 to -4.06 percent, calculated as the antilog of the parameter estimate for Dunedin plus or minus twice the standard error, or [antilog ((-.05816)$^{+}/_{-}$ (2 * .00838))] - 1 = (-.0722,-.0406).

The parameter estimates for the price index and city location dummy variables are simply percentage adjustments (multipliers) to the predicted sale price of a single-family dwelling in Palm Harbor in 1984. These multipliers are obtained by taking the antilog of the appropriate coefficient. The parameter estimates for the TOTLIV, SQFTCOS, and SQFTLOT variables, likewise, are adjustments to sale price but vary depending on the square footage of conditioned living space, cost per square foot of land, and square footage of the lot. The interpretation of the parameter estimates can be illustrated in the context of the following example.

Recall the model is of the form,

$\log Y = a + b_iX_i + error_i,$ where $i = 1,\ldots,21.$

Equivalently

$$Y = e^{(a + b_iX_i + error_i)} = (e^a)(e^{b_iX_i})(e^{error_i})$$

$$= (e^a)(e^{b_i})^{X_i}(e^{error_i})$$

which can be rewritten

$$Y = (a')(b_i')^{X_i}(c')$$

where

$a' = e^a;$

$b_i' = e^{b_i};$

$c' = e^{error_i},$ but $E(error_i) = 0$, therefore, $c' = 1;$

X_i = any value for TOTLIV, SQFTCOS, and SQFTLOT (X_1, X_2, and X_3),

0 or 1 for the dummy variables ($X_4 \ldots X_{21}$).

Therefore, the predicted price of a new, single-family dwelling in Palm Harbor in 1984 with 1654 square feet of living space, a land cost of $2.17 per square foot, and a lot size of 16,200 square feet is

$$Y_{Pred.} = e^{(10.0379155)}e^{(.0003904)(1654)}$$
$$e^{(.1782586)(\$2.17)}e^{(.0000086)(16{,}200)}$$
$$e^{(0)}e^{(0)}$$
$$= (\$22{,}878)(1.9073629)$$
$$(\$1.4722932)(1.1494919)(1)(1)$$
$$= \$73{,}850.$$

The values in this example for living space, land cost, and lot size are the mean values for these variables for Palm Harbor in 1984. Of course, for any specific dwelling the appropriate values would be used instead.

Because Palm Harbor is the base city and 1984 is the base year of the price index, the price index and city multipliers are relative to the predicted price of a single-family dwelling in this city in 1984. Thus, the time and city adjustments in the above equation are zero and result in a multiplier of one for both time and location. If, however, the predicted price of a single-family home in Dunedin in 1975 with the same characteristics (square footage of living space, land cost, and lot size) is desired, the appropriate multipliers would be found by taking the antilog of the coefficients for Dunedin (-0.05816044) and 1975 (-0.36253507) and multiplying by the price predicted above.

$$\begin{aligned} Y_{\text{Pred.}} &= \$73,850(e^{-0.0581604})(e^{-0.3625351}) \\ &= \$73,850(.9434986)(.6959099) \\ &= \$48,489. \end{aligned}$$

Negative parameter estimates, of course, result in a multiplier less than one, while positive parameter estimates result in multipliers greater than one. The price predicted by the model for a single-family dwelling in Dunedin in 1975, adjusted for time and location, is therefore, $48,489, about 34 percent less than that for the same home in Palm Harbor in 1984.

Upon completion of the first regression, studentized residuals were calculated for each observation and a plot of the statistics printed. The decision was made to eliminate any observation with a studentized residual lying more than plus or minus two standard deviations from zero. This was done as a final screening of the data to delete any observations still in the dataset that may have represented a land sale or data entry error. This procedure resulted in 538, or 5.73 percent (538/9,395) of the total observations not subject to impact fees, being eliminated from the study. The large sample size permitted a relatively liberal policy with respect to eliminating outliers, and resulted in more stable estimates of the model coefficients.

Table 4 summarizes the annual number of new home sales by city after deletion of the 538 observations having studentized residuals falling outside the region of acceptability.[4] From Table 4 it can be seen that the deletion process resulted in about 1.2 percent of the observations for Dunedin being eliminated [(1,090-1,077)/1,090 = .0119] and about 3.9 percent of the observations for Clearwater being eliminated [(2,782-2,674)/2,782 = .0388]. Again, because of the large sample size the deletion of these data did not adversely effect the methodology employed in this study.

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[4] The chi-square test was performed again after deletion of the outliers. The total number of new homes in Dunedin increased from 87 in 1973 to 119 in 1974, while the total number of homes built and sold in the other cities decreased from 371 in 1973 to 294 in 1974. The value obtained from the chi-square calculation (11.59) once more leads to rejection of the hypothesis of independence at the .001 level.

Table 4

Annual Sales of Single-Family Dwellings by City After Deletion of Outliers.

	Dunedin	Clearwater	St. Petersburg	Safety Harbor	Palm Harbor	
1970	5	78	59	0	0	142
1971	31	96	88	0	0	215
1972	109	141	136	1	0	387
1973	87	192	120	21	38	458
1974	119	157	106	14	17	413
1975	50	149	59	0	23	281
1976	32	153	57	10	80	332
1977	114	234	50	75	111	584
1978	96	292	125	120	336	969
1979	139	463	151	100	427	1,280
1980	103	232	92	170	541	1,138
1981	69	170	88	192	421	940
1982	36	94	18	140	316	604
1983	48	139	50	166	554	957
1984	39	84	58	202	673	1,056
Total:	1,077	2,674	1,257	1,211	3,537	9,756

The second step of the analysis involved fitting the model to the remaining 8,857 observations on new home sales not subject to impact fees (9,395 - 538 = 8,857). The results are illustrated in Table 5.

The second regression yielded an R^2 of .9539, an increase of .068 from .8858, an F value of 8,697.26 (significant at .0001), and a root mean square of .0834. It is obvious, and no surprise, that the data trimming results in the enhanced power of the model to explain price variability.

Table 5

Regression Results--Second Run.

Independent Variable	Parameter Estimate	Standard Error	T-statistic
INTERCEPT	9.94193296	0.00611463	1625.93
TOTLIV	0.00047202	0.00000244	193.54
SQFTCOS	0.17025784	0.00169767	100.29
SQFTLOT	0.00000899	0.00000014	63.52
YR1	-0.75229712	0.00861111	- 87.34
YR2	-0.70690496	0.00752471	- 93.94
YR3	-0.63255503	0.00654294	- 96.68
YR4	-0.52979273	0.00599302	- 88.40
YR5	-0.47471432	0.00612633	- 77.49
YR6	-0.42245508	0.00663091	- 63.71
YR7	-0.42284083	0.00621457	- 68.04
YR8	-0.38837158	0.00546653	- 71.05
YR9	-0.31009937	0.00449816	- 68.94
YR10	-0.20328744	0.00412336	- 49.30
YR11	-0.12957097	0.00392378	- 33.02
YR12	-0.08282852	0.00406044	- 20.40
YR13	-0.08627458	0.00449292	- 19.20
YR14	-0.06789882	0.00410083	- 16.56
DN	-0.02864388	0.00527663	- 5.43
SP	-0.08137364	0.00315908	- 25.76
CW	-0.02142658	0.00263983	- 8.12
HR	-0.00026477	0.00299001	- 0.09*

* Not significant at alpha = .05.

The price index continues to behave as expected with large, negative parameter estimates from 1970 (-0.7523) increasing each year to a parameter estimate of (-0.0679) in 1983. The t statistic for each coefficient in the price index is significant at alpha = .05. The parameter estimates for the city dummy variables cluster more closely after deletion of observations with studentized residuals falling outside the bounds of acceptability. It is likely that one or more extreme sales in each city were affecting the coefficients in the first run. Thus, with their elimination the parameter estimates in the subsequent run are more closely grouped together.

Note that while the regression coefficient for Safety Harbor is no longer significant at alpha = .05 this is of little import as the significance of the city dummy variables is a function of which city is chosen as the base city. If some other city were chosen instead of Palm Harbor, say St. Petersburg, then the regression coefficient for Safety Harbor may have been positive or negative and/or significant or nonsignificant, while the coefficients for the remaining city dummy variables would have changed as well.

The third step of the analysis involved calculating the mean ratio of actual sale price to predicted sale price for the data subject to impact fees (899 observations). The regression results of the second step were applied to the data on new home sales subject to impact fees in order to determine predicted price. Since the database includes the actual sale price for all observations, it was then simply a matter of comparing the two values.

Using the SAS univariate procedure, it was found that the mean of the ratio of actual, new home sale prices in cities with impact fees to that predicted was 1.0312 with a corresponding standard error of .0033 percent. The standard error of the mean, a measure of the accuracy of the sample mean as an estimate of the unknown population mean, is calculated by dividing the estimated standard deviation of the population by the square root of the number of observations in the sample of new home sales with impact fees. Thus, if the model is correct, the mean of the actual sale price of new, single-family dwellings subject to impact fees was between 2.46 and 3.78 percent[5] higher than predicted based on the sale of comparable housing not subject to fees during the period of this study. Table 6 summarizes some of the statistics resulting from the univariate procedure.

Table 6

Ratio Statistics.

	CW	DN	TOTAL
N	195	659	854
MEAN	1.029790	1.031660	1.031240
STD DEV*	0.124093	0.086179	0.096083
VARIANCE	0.015399	0.007427	0.009231
STD ERROR**	0.008886	0.003357	0.003287
T: MEAN = 0	115.883000	307.309000	313.645000
PROB>T	.000100	.000100	.000100

* Standard Deviation. ** Standard Error of the Mean.

The univariate procedure with the plot and frequency options was chosen in lieu of other SAS procedures which generate descriptive statistics because of its ability to provide greater detail on the distribution of the variable of interest, in this case the variable ratio. In addition to the number of observations, mean, variance, and standard deviation, the univariate procedure also provides details on extreme values, quantiles, various plots depicting the distribution of the variable, frequency tables, and a normality test of the data (SAS User's Guide: Basics, 1985).

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[5] The approximate 95 percent confidence interval calculated as ((3.12 $^{-}/_{+}$ (.0033 * 2)).

Note the total sales of new homes subject to impact fees is now 854 instead of 899. Initial examination of the plots produced by the univariate plot option revealed the existence of 45 observations with values more than three interquartile ranges below or above the sample 25th and 75th percentiles when plotted using a box diagram. An interquartile range is the distance between the 25th and 75th sample percentiles.

The stem-and-leaf plot (Tukey, 1977), printed when the number of observations falling into a single interval is no more than 48, and the horizontal bar chart, printed when the number of observations is greater than 48, both confirmed the existence of these extreme values. In most instances these values represented isolated points as opposed to clusters. A clustering of points representing extreme values would have been cause for concern and most likely mandated a different method for screening the data.

Finally, the normal probability plot of the variable ratio which plots the empirical quantiles against the quantiles of a standard normal distribution revealed these observations to lie far from the reference line--calculated from the sample mean and standard deviation. The decision was made to drop these 45 observations from the dataset. Because the previous data screening techniques only applied to data which were not subject to impact fees, it was felt this final trimming of the data was justified in order to delete any observations in this data subset that were actually land sales or that may have represented a nonarm's length transaction. These 45 observations represented 5.01 percent (45/899) of the dwellings subject to impact fees. Once again the large dataset used in this study afforded the luxury of being very conservative in deciding which data would be retained for analysis.

The SAS program was revised to delete any observations with a ratio less than .7 or greater than 1.35 prior to calculating the moments generated by the univariate procedure. The bounds were those necessary to eliminate values in the extreme lower and upper tails of the distribution. The deletion of the 45 observations resulted in a more robust measure of the parameters and reduced standard errors.

Referring once again to Table 6, in addition to descriptive statistics for the individual cities, weighted average statistics based on the number of observations in Clearwater and Dunedin are provided. As expected, Dunedin, with more than three times as many observations as Clearwater, clearly dominates the aggregate calculations. The mean of the ratio of actual sale price to predicted sale price is 1.0298 and 1.0317 in Clearwater and Dunedin, respectively; a differential of approximately three percent. The measures of dispersion--variance and standard deviation--are greater for Clearwater than Dunedin, but not enough to warrant concern. Greater variability in the Clearwater sample is likely a function of highly volatile prices in the two years subsequent to the imposition of impact fees. While Dunedin was subject to the same volatility in 1983 and 1984, the time frame with respect to the duration of fees was much greater and therefore the model would be expected to predict prices with less variability in Dunedin than Clearwater.

The standard error of the mean for Clearwater, however, is more than 2.5 times as great as that for Dunedin. Because fewer observations exist for Clearwater, the accuracy associated with the sample mean as an estimate of the true population mean is less. Calculated as the estimate of the population standard deviation divided by the square root of the number of sample observations, the standard error of the mean is a decreasing function of the number of observations in the sample. Note that if the number of observations was the same for both cities and the standard deviation remained the same, the standard error of the mean in Clearwater would have been only 44 percent greater than the same statistic for Dunedin, calculated as [((.124093/square root of 659) - (.003357)) / .003357] = .439969.

Finally, the observed mean ratio with a significance level of p = .0001 implies chance is not a plausible explanation for the difference between actual and predicted sale price in the sample of data for which the mean ratio statistic is calculated. Thus, it can be concluded with a high degree of certainty that actual sale prices in Clearwater and Dunedin were greater than those predicted by the model for years in which new home construction was subject to impact fees. The model, which was developed using data on new home sales not subject to impact fees, on average, underestimates the actual sale price of those homes subject to an impact fee. Therefore, it appears that builders in Dunedin and Clearwater during the period of this study were passing on the cost of impact fees in the form of higher prices. It should be emphasized that the model consistently underestimates predicted versus actual sale price in both cities, not just in the aggregate, even though the time period over which fees were in effect was nine years in Dunedin and two years in Clearwater. This would tend to further support the validity of the methodology employed in this research to isolate both the short-term and long-term price effects of impact fees.

CHAPTER V
SUMMARY AND CONCLUSIONS

Modification of the original model and a change in the research methodology made it possible for this study to accomplish its primary objective--an examination of the incidence and price effects of impact fees on a selected housing type in one Florida housing market. While the revised model was constructed from the same theory underpinning the development of the original model, the methodology employed was quite different from that originally proposed. Regrettably, the changes which were necessary to overcome certain statistical problems prevented testing for the indirect effects of impact fees upon house prices in cities without fees.

While precluded from testing for the indirect effects of impact fees, it must be stated that the sale price of new, single-family dwellings in nonimpact fee cities may have been affected. In consideration of the theory discussed in Chapter II, it is likely that prices of new homes in nonimpact fee cities were higher than they would have been in the absence of impact fees. If subsequent research finds this to be true, then the price effect of impact fees on dwellings in Dunedin and Clearwater from

1976 and 1983, respectively, was actually greater than indicated in this study.

The question remains--are impact fees capitalized into the price of new homes in other cities located in the same housing market but which do not have impact fees? A topic for future research, this question would be a logical extension of the work completed in this project. The methodology employed in this study offers a potential framework to compare actual and predicted sale prices in nonfee cities prior to and subsequent to the imposition of fees in nearby cities.

Another area of research which might evolve from this study is an examination of trends in land prices in cities where builders have been subject to impact fees for extended periods of time. Builders may adjust to higher costs resulting from impact fees by reducing other factor costs, most notably land costs. It is conceivable that builders eventually compensate to some extent through lower bid prices for land. A comparison of raw land prices before and after the imposition of fees may provide some evidence as to whether impact fees are indeed being capitalized into lower land prices. Ideally, prices would be compared on a pre- and postfee basis in a city with impact fees to prices for land in a city without fees. With respect to this research, it may be that new home sale prices in Dunedin actually were less than they otherwise would have been if it is assumed builders were able to pass backward some portion of the costs attributed to impact fees.

The research documented in this dissertation represents one of the first empirical attempts to determine the incidence of impact fees and the degree to which impact fees are capitalized into the sale price of new, single-family dwellings. While it cannot be said with absolute certainty that impact fees are responsible for higher prices, the major conclusion supported by the data in this study is that impact fees are significant and are being paid by new homebuyers in the form of higher prices. Thus, in the Pinellas County housing market, assumed to have a relatively elastic supply of, and inelastic demand for, new housing, the empirical evidence supports the hypothesis that most, if not all, of an impact fee is passed forward to new homebuyers in the form of higher prices. The mean value of the actual sale price of new, single-family dwellings subject to impact fees was 3.12 percent greater (.0033)[1] than that predicted by the model based on sales of comparable housing.

This conclusion differs from the findings of another study which found development fees do not significantly influence housing prices (Zorn, Hansen, & Schwarz, 1986). A possible explanation for this discrepancy is that impact fees of more than a few hundred dollars have not been widely imposed until recently. Further, there has been a lack of sufficient data to study trends in new home sale prices in multiple cities over time where some cities impose impact fees and others do not.

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[1] The number in ( ) is the standard error.

The revised model performed well considering events which occurred during the years encompassed by this study (1970-1984)--two major cycles in building construction, inflationary pressure in the late 1970s, and a highly volatile mortgage market from 1980 through 1984. As constructed, the model was able to account for changes in new home prices resulting from any of these factors. The model, however, was not equipped to deal with price variability occasioned by a change in the relative attractiveness of cities with impact fees compared to cities without impact fees subsequent to enactment of a fee ordinance. The assumption was made that any differences in consumer demand for housing among cities remained constant and was not affected by the presence or absence of impact fees. The shorter the time frame the more palatable is this assumption. Over longer periods of time, the validity of the assumption becomes more suspect.

In light of the possible time-sensitive nature of this assumption, future research should focus on how the provision of capital improvements influences homebuyer perceptions of value. This would necessitate devising a measure of consumer preference based on the perceived adequacy or desirability of certain capital improvements; specifically, those easily identifiable improvements such as new schools, parks, and improved transportation corridors as opposed to improvements such as sewers and water mains which are equally valuable but less obvious. The model could be modified to include this measure and used to ascertain whether it is the actual cost of impact fees causing new home prices to be higher, or whether it is increased demand for housing in cities with adequate infrastructure that is responsible. The answer to this question may strongly influence local decisions regarding not only what public services are to be financed with impact fees, but the timing of the construction of the services as well.

The database used in this study deserves some comment. Admittedly large and cumbersome and not entirely free of error, this database, nevertheless, is an excellent resource for researchers interested in not only residential housing problems, but commercial, industrial, and agricultural issues as well. The database, which classifies each parcel according to one of a hundred land use types, should prove useful to those interested in price trends and valuation of different property types.

While the database has certain limitations, its major attraction from a research standpoint is the current and comprehensive information it contains. Information is maintained on every property parcel in a county and is updated on an annual basis by the county property appraiser's office. Although access to a mainframe computer is required to extract data from these tapes (both the IBM 3090 and 4341 were used in this study), it is surprising that research reported in the literature has generally failed to use this data source.

Finally, the database affords researchers the opportunity to test hypotheses regarding the economic incidence and price effect of impact fees on existing housing stock as well as new home sales. It may be possible to ascertain whether impact fees are contributing to higher sale prices of existing housing and thus to windfall profits on the part of current homeowners. The sale price of existing dwellings may be rising as a direct result of the higher cost of new construction or because demand is increasing for all types of housing in the community assessing impact fees for capital improvements. Regardless, the model developed for this study and the methodology employed would appear to be a logical place to start in the investigation of issues such as this.

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BIOGRAPHICAL SKETCH

Charles J. Delaney was born May 24, 1952, in Greenfield, Massachusetts. He was raised in Western Massachusetts and graduated from the University of Rhode Island in June 1974 with a bachelor's degree in economics. He earned a master's degree from the University of Florida in August 1981 in extension and adult education.

From 1977 to 1987 Mr. Delaney was employed by the Cooperative Extension Service in Massachusetts and Florida where he was responsible for the development and implementation of energy education programs targeted to the commercial (small business) and residential sectors. During that time Mr. Delaney obtained and administered more than $2,000,000 in grants to support the development of educational programs and curriculum and to promote alternative energy research and applications

Mr. Delaney is married to the former Judith Jopling of Lake City and Ft. White, Florida, and has one son, David. He currently is conducting research at the Real Estate Research Center in the College of Business at the University of Florida.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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